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A Political Meaning of "Scientific Philosophy"? The Case of Edgar Zilsel

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Résumé : Aujourd'hui, bon nombre de philosophes des sciences ou d'universitaires semblent penser que leur expertise peut éclairer les débats publics. Le premier empirisme logique peut apparaître comme un modèle de philosophie des sciences politiquement pertinent. Dans ses travaux sur la « dépolitisation » de l'empirisme logique, George Reisch a aidé à prendre conscience de l'agenda politique (de certaines composantes) du Cercle de Vienne, agenda qui a disparu dans les États-Unis d'après-guerre, sous la pression de l'anti-communisme. L'étude du cas d'Edgar Zilsel, un sociologue des sciences membre de l'aile gauche radicale du Cercle de Vienne, montre cependant que l'empirisme logique ne peut pas être considéré aussi facilement comme politique. Certes, Zilsel était un intellectuel politiquement engagé, mais il n'était pas un philosophe des sciences politiquement engagé : son intention n'était pas d'intervenir dans les débats publics ou de soutenir des causes publiques en vertu de son statut académique (il n'en avait pas) ou de son expertise professionnelle (il en avait une). Je soutiens en outre que les débats contemporains sur la place de la science dans la société, menés par des universitaires qui, soucieux de leur impact sur les questions de société, conceptualisent l'influence sociale en termes de motifs et de valeurs individuels, ne correspondent pas au point de vue des empiristes logiques de gauche : ceux-ci cherchaient à fournir une explication sociologique de la science et s'attachaient davantage aux déterminants structuraux qu'aux motifs individuels. Ainsi, même si l'empirisme logique peut être vu comme un modèle de philosophie des sciences politiquement pertinent, il pointe, à mon sens, dans une direction différente de celle généralement prise par ses héritiers d'aujourd'hui.

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Abstract: Many scholars in the philosophy of science and the HOPOS community today appear to be keen for their expertise to be relevant to public concerns and debates. Early logical empiricism appears to provide a model of politically relevant philosophy of science. In his studies on the "depoliticization" of logical empiricism, George Reisch has rekindled wider awareness of the political agenda of (some sections of) the Vienna Circle, an agenda that faded away in the post-war US only under the pressure of anti-communist repression. In my study of the case of Edgar Zilsel, an early sociologist of science and politically radical member of the left wing of the Vienna Circle, I will show that logical empiricism cannot so easily be said to have been political. To be sure, Zilsel was a political intellectual—but he was not a political philosopher of science, i.e., his intention was not to intervene in public debate or to act in support of public causes by virtue of academic status (which he did not possess) or professional expertise (which he did). I shall argue further that contemporary debates about science in its social context conducted by scholars who are concerned about their impact on broader societal issues and who conceptualize social influence in terms of individuals' motives and values are at odds with left-wing logical empiricism: this sought to provide a sociological account of science and focused on structural determinants rather than on individuals' motives. Thus, even if logical empiricism could be shown to be a model of politically-relevant philosophy of science, it would point, I suggest, in a different direction to the one generally discussed by its contemporary heirs.

1 Introduction: A socialist agenda "at the heart of" logical empiricism?

In his seminal book, *How the Cold War Transformed Philosophy of Science* [Reisch 2005], George Reisch portrays the rise of the philosophy of science in its contemporary form as the result of a process of "depoliticization". In telling this story, Reisch is pursuing a twofold aim. First, as a trained historian, he seeks to present a fairly obscure but intriguing chapter in the history of philosophy of science to a wider public: the story of how logical empiricism came to be depoliticized during the cold war due to the pressure of anti-communism and became what is nowadays known as the "philosophy of science", an academic discipline committed to professionalism, specialization, political quietism, and formalism, i.e., to the idea "that a scientific theory can be specified as a formal system resting on axioms" [Reisch 2009, 194]. In addition to this, however, Reisch explicitly states that he has a further agenda, making no secret of the fact that he mourns the loss of a politically engaged philosophy of science. American anti-communists, he writes, "helped kill the

last century's best hope for a serious, politically engaged philosophy of science in North America" [Reisch 2009, 193].

Reisch is not out on a limb with this agenda. On the contrary, it seems that there are many US scholars who share his sense of loss. In a comment on Reisch's work, Don Howard expresses a fairly similar view:

> [B]y the end of the 1950s, thoughtful philosophical debate about the place of science in society had all but disappeared, replaced by a highly formalized philosophy of science pursued by a new generation of technically well-trained young specialists whose inability to think carefully about science in context was disguised as disdain for irrelevant, non-technical questions. [...] The story that Reisch tells is also a tragedy. The tragedy is that postwar philosophy of science lost the will and the ability to theorize the social role of science. [Howard 2009, 199, 201]

Howard goes on to ask "what is necessary in a theory of science adequate to the task of empowering philosophers of science to participate in public debate about science in a social context". In summing up Reisch's contribution, Alan Richardson concludes that he "recovers the socialist agenda [...] at the heart of logical empiricism", stating explicitly:

> Reisch's book might embolden philosophers of science to begin anew their debate about the kind of contribution they should make to public discourse and political action. [Richardson 2007, 360]

Thus, a number of influential scholars have expressed a wish to engage in public debate—or at least to "think carefully about science in context" and to "theorize the social role of science". It is clear, furthermore, that they wish to do this *qua* philosophers or historians of science, i.e., not in their free time as ordinary citizens but as recognized university employees, a role which brings with it certain privileges including salary, time, prestige and access to the media. There is another item in this conception of a "political philosopher", I suggest, which merits an explicit mention: political intervention *at no personal risk*.

The attitude expressed by the afore-mentioned scholars may be surprising to readers based in continental Europe, and indeed there seems to be something peculiarly American (or at least Anglo-Saxon) about it. Philosophers and historians of science in the US teach at liberal arts colleges, and the education these colleges traditionally offer, in contrast to a professional, technical, or economic curriculum, aims at producing *better citizens*. This does not mean that students are to be indoctrinated, of course, but neither is it restricted to merely equipping them with the faculty of critical reflection. The idea of a liberal education, often traced back to John Henry Newman's *The Idea of a University* [Newman 1905], is to produce a "habit of mind [...] which lasts through life, of which the attributes are freedom, equitableness, calmness,

moderation, and wisdom". This, according to Newman, is nothing less than the "main purpose of a University in its treatment of students" [Newman 1905, 101–102]. It has been noted that this ideology is under threat today from a "growing emphasis on financial success as the singular goal of higher education, exemplified by the rise of for-profit colleges run on business models" [Nagelsen 2014, 145]. Philosophers of science who observe their discipline's commitment to professionalism, specialization, political quietism, and formalism, as Reisch put it, may be concerned about what they can contribute to a traditional liberal education despite these trends. Perhaps this is the context in which the comments quoted above, all of which express concern regarding the political engagement of philosophy of science, should be situated. According to Reisch's analysis, early logical empiricism promises to show what political engagement in the philosophy of science might look like today—to which my response is: does it?

The aim of this paper is to examine whether logical empiricism provides a model of politically engaged philosophy of science. To be clear from the start: I think it does not. My starting point is a twofold conceptual weakness in Reisch's approach. (By "weakness" I mean a conceptual 'default' setting which is due to Reisch's particular research interest but is not appropriate for carving out the alleged political dimension of logical empiricism.) Concentrating on the story of logical empiricism's "depoliticization", Reisch fails to point out in what sense it was political before this time. How were philosophy and politics interrelated in logical empiricism, and what might it mean to claim that there was a socialist agenda "at the heart of logical empiricism", as Alan Richardson puts it? This question remains unanswered (as Richardson also notes), though one could imagine a variety of answers. Did (some) members of the Vienna Circle simply hold certain political convictions alongside their philosophical convictions? Did they try to harmonize the two sets of convictions, to merge them into a coherent whole? Or, finally, did they think of their philosophical attitude as a political one? It seems to me that what Reisch (and also Howard) have in mind is something close to the latter version, which is the most ambitious one. I think instead that the second answer is the correct one.

There is a further reason, though, why readers of Reisch's book will find hardly any conceptual resources in it for tackling this question on their own. Given the story he wants to tell, it is quite natural that Reisch should look at the Vienna Circle refugees arriving in the US through the eyes of those Americans (mostly New York intellectuals) who welcomed them. Reisch provides a reliable portrait of this intellectual milieu of the 1930s, using a convincing and useful categorization that reconstructs the main strands of political discourse of the time according to four camps:

1. the liberal pragmatic left
   exemplified by John Dewey and Ernest Nagel, "liberal, democratic, and socialist-friendly pragmatics in New York City", who shared a "commitment to scientism" and embraced science "as the epitome of

knowledge and a tool to enlighten the public, improve modern life and secure modernity" [Reisch 2005, 58–59];

2. the socialist left
exemplified by the journals *Partisan Review* (and in particular William Gruen, who introduced logical empiricism to its readers) and *Philosophy of Science*; "generally more committed to Marxism (in some form), they took central themes of Marxism, such as class struggle and social and economic planning, to be crucial concerns for any adequate philosophy of science" [Reisch 2005, 59];
3. the radical academic left
exemplified by the journal *Science & Society*; scientists, philosophers and historians "more convinced of the basic truth of Marxist theory", who consider science to be "a powerful, potential tool for social progress" if "coupled to socialist politics and a metaphysics of dialectical materialism"; "[m]any, therefore, could not abide logical empiricism's rejection of metaphysics" [Reisch 2005, 60];
4. the communist left
exemplified by *The Communist*; the "most extreme group of philosophers on the left", united by their orthodox reading of Marx, Engels and Lenin and their rejection of any "creative interpretation on the part of intellectuals"; these figures identified philosophical practice with communist party life [Reisch 2005, 61].

If we are interested in reconstructing the interrelationship between philosophy and politics within logical empiricism, however, this classification is not very helpful for two reasons. First, these four camps are constructed according to a measure of strength of commitment, ranging from "socialist-friendly" to "orthodox". The crucial question for us is: commitment to *what* exactly? As we shall see later on, the Marxist camp was expansive enough to embrace not only differences in degree but also serious disagreements about basic philosophical questions. Second, and also very important in this context, the classification is designed in terms of understanding the *reception* of logical empiricism, not its *origin*. The two parts of the story, i.e., politicization, on the one hand and reception and depoliticization on the other, took place in completely different socio-cultural contexts and therefore need to be told in different categories. If logical empiricism was political, it was forged within Europe's Marxist debates; it is thus to these that we should pay attention when seeking to understand the political views of the left wing of the Vienna Circle.

Both points together serve to shift some details of Reisch's story into a different perspective. Let me briefly offer one striking example. The fact-value dichotomy, which appears in Reichenbach as the famous distinction between "context of discovery" and "context of justification" and then became fundamental to the thinking of so many 20th century philosophers of science,

is understood by Reisch as a strategy pursued by the right wing of logical empiricism in order to theorize its own depoliticization [Reisch 2005]. This sounds good—perhaps even too good to be true. Indeed, this cannot be the whole story, because in point of fact, as we will see later, the left wing of logical empiricism also adhered to the fact-value dichotomy. (In particular Edgar Zilsel, on whom I will focus in this paper, accepted this dichotomy, despite everything in his work which seems *to us* to hint in the opposite direction. As we will see, the fact-value dichotomy was deeply rooted in the peculiar kind of Marxism that dominated Zilsel's Vienna.)

In this paper, I will reconsider the claim of a "political epistemology" of logical positivism, only this time from the perspective of the Vienna Circle's political context prior to emigration. To begin, I will give a rough overview of Marxist debates in pre-war Europe (at least in the German-speaking parts), debates to which the founding members of the Vienna Circle were exposed and which framed their political thinking. While the important influence of neo-Kantianism and pragmatism on logical empiricism is widely acknowledged today, the impact of Marxism may still be underestimated. We shall see, in particular, that even in the relevant subset of varieties of Marxism there is enough room for contradictory views concerning the topics that are of importance in the philosophy of science. Merely confessing allegiance to "Marxism" could thus hardly be the defining criterion of the various epistemological approaches adopted. In the section that follows, I will take a closer look at the case of Edgar Zilsel. Zilsel is particularly interesting because politics and philosophy seem to be inextricably intertwined in his work. Furthermore, Zilsel is even less known than Reisch's hero Neurath. He is virtually absent from Reisch's book and yet certainly deserves attention as an original thinker. As already mentioned, the results of this analysis will prove to be more in the negative: even in the case of Zilsel, it is hardly possible to speak of a political philosophy of science.[1] In the concluding section, I will show briefly that those in the current debate who seek to restore some form of social relevance to the philosophy of science are in any case somewhat at odds with what comes closest to a political reading of scientific philosophy.

2 "Marxism" around the Vienna Circle

Before tackling the main question of this paper, we should briefly recall the "big divides" in Marxism and identify the kind of Marxism to which the Vienna Circle was exposed. The fact that Marxism might be relevant per se for understanding logical empiricism surely comes as no surprise today. Recent work by Reisch and also by Uebel [Uebel 2004, 2005] concerning a "left wing" of the Vienna Circle has enjoyed a widespread and positive

1. Sarah Richardson came to similar results for Carnap and Neurath [Richardson 2009a].

reception. To be sure, given the wider cultural and intellectual setting of the German-speaking world of the 1920s, it is a simple truism that the modernism of the "scientific world conception" incorporated a political dimension. "Politically, there was a clash between the democratic alliance of the new scientific philosophers like Carnap, Reichenbach, Schlick, Neurath and, directly opposed, the conservative, neoromantic, and anti-socialist faction," write Raven & Krohn in [Zilsel 2003, xli]. In this sense, the political dimension of scientific philosophy was induced by its unprogressive environment, so it is not surprising that this political stance faded in the context of American exile.[2] Even if Carnap, for example, did not drop his political convictions, there was no longer any reason for him to merge them with his philosophical approach or to think of the latter as a political stance.

But there is also the case of those who resisted the process of depoliticization in the US and who subsequently paid the price of being marginalized. Reisch comments in detail on the cases of Otto Neurath and Philipp Frank. In this paper, I add to these the name of Edgar Zilsel. What these figures have in common is that their political engagement prior to emigration was not only "induced" in the sense explained above but can indeed be considered to be more substantial. These left-wingers were engaged in the Vienna Movement for Adult Education, they were close to the relatively militant Austrian social democratic party, and they happened to label themselves Marxists.

So what did it mean to be a "Marxist" in the days of early logical empiricism? For the sake of brevity, I will attempt to present the varieties of Marxism in pre-war Europe as schematically as possible. Without oversimplifying, it can probably be said that there were three main strands of Marxism during the early 20th century:

- "Orthodox Marxism", i.e., the "vulgar" reading of Marx that prevailed in the Second International, encompassing a belief in deterministic development towards socialism; Dialectical Materialism appears within this as the official doctrine of the Bolshevik party, based on Lenin's *Materialism and Empiriocriticism* [Lenin 1908];
- Hegelian Marxism, in particular Lukács and the critical theory of the Frankfurt School;
- Ethical socialism (of neo-Kantian provenance) and its "Austro-Marxist" version.

The first type of Marxism listed above is of less interest to us here, because it had no considerable impact on the *formation* of the left wing of the Vienna

2. This environment also explains the "(partial) convergence between liberalism and social democracy", which Romizi identifies as the background to the alleged "Vienna Circle's political engagement" [Romizi 2012, 222]. As I try to show in this section, the divides *within* Marxism are much more informative for discussing the claim of a "political philosophy" of the Vienna Circle than these ephemeral *external* coalitions. If this is correct, it would also follow that Romizi's harsh criticism of Sarah Richardson is unsubstantiated [Richardson 2009a,b].

Circle. Nonetheless it was, of course, highly influential and thus crucial to the fate of Marxism in the 20th century. Furthermore, it probably also played an important role in the *reception* of the Vienna Circle in the US, given its influence on both the "radical academic left" and the "communist left" in Reisch's classification.

The remaining two types on the above list, however, can be said to span the range of alternatives which are crucial for understanding the relation between philosophy and politics in the left-wing Vienna Circle: On the one hand Hegelian Marxism (e.g., Lukács in the 1920s, early critical theory, Herbert Marcuse) which put an emphasis on the critique of ideology with a view to setting off revolutionary change; on the other hand ethical socialism (Karl Vorländer at Marburg) and Austro-Marxism (Max Adler at Vienna) which adhered to historical determinism and thus favoured a more positivist reading of historical materialism, but had the problem of reconciling the determinist conviction with the idea of political intervention (hence especially the importance of the fact-value dichotomy which permitted Vorländer to reduce the Marxist conviction to an "ethical attitude").

Hegelian Marxism and Austro-Marxism can be thought of as crystallizing around two divergent notions of "practice". "Practice", indeed, is a central notion in the early writings of Karl Marx, which were published posthumously in the late 19th and early 20th century. These posthumous editions were of utmost importance to heterodox Marxist currents of the 1920s, for they seemed to unveil aspects of Marx that were barely compatible with the orthodox reading of the Second International: the early Marx seemed to show a stronger ethical attitude and placed greater emphasis on the phenomenon of alienation. In his *Theses on Feuerbach* from 1845, published for the first time in 1888 [Marx 1888], Marx outlined an approach to knowledge that challenges any clear distinction between "theory" and "practice". In *The German Ideology* [cf. in particular Marx & Engels 1926], co-authored by Marx and Engels in 1846 and firstly published between 1905 and 1932, the authors suggested a reading of history as determined by material and economic "practice". Rather than elaborating on the countless attempts to spell out both theories, I shall merely indicate two different and even opposing meanings of the term "practice" presented in these writings, namely

- practice in the sense of political action, in particular revolutionary action;
- practice in the sense of labour or the material reproduction of society, and hence, on the contrary, "conservative" practice, where "conservative" is to be understood not in political terms but in the sense of a functionalist sociology.

The first sense prevails in the *Theses on Feuerbach*, while the latter one dominates the quite empirical and empiricist approach of *The German Ideology*. Starting from this ambiguity it is possible to develop a schematic

overview of the remaining two strands of Marxism, ethical socialism and Austro-Marxism on the one hand and Hegelian Marxism and critical theory on the other. This is what I seek to do in the following table, where I characterize both strands according to the following points: (1°) a reading of "practice", (2°) a reading of "historical materialism", (3°) philosophical consequences, and (4°) ideal or guiding model of politics:

	Ethical socialism (neo-Kantian)/ Austro-Marxism	Hegelian Marxism/ Critical Theory
1	Practice as material reproduction of society	Practice as revolutionary change
2	Historical Materialism as a sociological approach to consciousness and to science	Critique of ideology/ false consciousness
	(symmetrical)	(asymmetrical)
	critical in the sense of revealing the social roots of science	critical in the sense of revealing class interests and power relations
	reflexivity: science of science[3]	non-reflexive (privileged standpoint thesis)
3	Fact-value dichotomy	No fact-value dichotomy
	Determinism, scientism, plus ethical supplement	Dialectics
	theory as (social) science/ politics as (social) engineering	Unity of theory and practice in the class consciousness of the revolutionary groups
4	Reform	Revolution

I owe this conceptualization to the illuminating analysis provided by Lucien Goldmann in his essay "Y a-t-il une sociologie marxiste?" [Goldmann 1957]. I have reframed Goldmann's insights slightly in the vocabulary of contemporary sociology of scientific knowledge, which characterizes itself as a "symmetrical" and "reflexive" approach.[4] "Symmetrical" means that it "would be impartial with respect to truth and falsity, rationality or irrationality, success or failure" and that it "would be symmetrical in its style of explanation", i.e., both sides of the dichotomies would be explained by the same types of causes. "Reflexivity" means that "[i]n principle its patterns of explanation would be applicable to [...] itself" [Bloor 1991, 7]. A traditional reading of the critique of ideology is, in this sense, *asymmetrical*, because it seeks a (causal) explanation only of "false consciousness" but not of

3. Or, even more specifically, sociology of sociology, cf. "Soziologische Bemerkungen zur Philosophie der Gegenwart" [Zilsel 1930], where Zilsel traces the origins of sociology back to a capitalist interest in predicting social and market trends.

4. Cf. the analysis in [Schlaudt 2018].

correct insight, and *non-reflexive*, because it relies on a privileged standpoint from where false consciousness can be correctly identified and criticized (the standpoint of the working class or of the revolutionary party).

The two columns of the above table should not be understood as exhaustive categories but rather as two opposing endpoints of a continuum filled with many "impure" cases. The Frankfurt School, for example, does not actually coincide with Hegelian Marxism but rather made some concessions to the opposite reading of historical materialism as a sociology of knowledge. By contrast, Edgar Zilsel may have accepted Austro-Marxism as a default setting before moving—perhaps under the pressure of rising fascist regimes in Europe—to the Hegelian side to some extent, giving more emphasis to revolutionary practice. (Thus although they started out from opposing positions, Zilsel and the Frankfurt School eventually came closer on certain points, and indeed Zilsel benefited from financial support by the exiled Institute for Social Research at New York.) Despite being very schematic, then, the main result to be gleaned from this classification is that an overall commitment to "Marxism" did not predetermine many of the controversial issues of interest to us in the context of the present paper. In particular, there was disagreement on the status and validity of the fact-value dichotomy, which was endorsed by ethical socialists and contested by Hegelian Marxists.

In my view, the left-hand column can be used as a good approximation of the kind of Marxism most influential for the members of the Vienna Circle insofar as they were Marxists. It is true that Max Adler, an important Austro-Marxist philosopher, modified ethical socialism in certain ways in order to resolve the tension between determinism and a voluntaristic concept of politics (he was thinking of a form of social determinism which takes the form of an ethical conviction in an individual's consciousness; cf. the analysis by Lucien Goldmann [Goldmann 1957]). It is worth noting, in particular, that the presence of pragmatic influences (perceptible especially in the late Adler as well as in Zilsel, as I will seek to show below) may have weakened the dichotomy between fact and value.

3 The case of Edgar Zilsel

3.1 Zilsel, logical empiricism and Marxism

I now turn to the case of Edgar Zilsel. Of the many biographical details contained in the numerous articles on his life and work,[5] I shall summarize those that are useful in relation to my analysis. Edgar Zilsel (Zilsel 1891-1944)

5. Stadler devotes some attention to Zilsel [Stadler 1997, 802–817], and there are also some informative studies on him by, for example, [Krohn 1985] and [Wulz 2012], a whole section on him in [Haller & Stadler 1993]. Also highly informative is Raven & Krohn's biographical introduction to [Zilsel 2003].

was a trained physicist, mathematician and philosopher. He attained his PhD at the University of Vienna with a dissertation on the law of large numbers, providing a formal and philosophical analysis thereof [Zilsel 1916]. Having completed his doctorate, he published on a wide variety of subjects, including thermodynamics, time and causality, in academic journals. He also served as a reviewer first and foremost for *Die Naturwissenschaften*, but also contributed a number of philosophical analyses to *Der Kampf*, the theory journal of the Austrian socialist party. In addition, he completed two monographs, both on "genius" [Zilsel 1918, 1926]. The first of these concentrates on the "cult of genius" and is more polemical in tone, though interspersed with attempts at a socio-historical explanation alongside epistemological considerations. The second monograph, which provides a reconstruction of the concept of genius in antiquity and in the early modern era, is much more academic and is rich in historical information. It is based on Zilsel's *Habilitationsschrift* which had been rejected by the University of Vienna. It may be of particular interest here that, rather than embarking on an academic career, Zilsel devoted his time and energy to adult education and to worker education in particular. If he thought of his teaching activity as political, then, this occurred in any case outside the university. As early as 1934, Zilsel lost his job after the Dollfuss putsch. Eventually, in 1939, he left Austria and moved to New York via London. In the US, supported by Horkheimer's Institute for Social Research and struggling to manage financially on diverse fellowships and grants, he published several now classic papers on the social origins of science collected in [Zilsel 2003]. He ended his own life in 1944.

Before turning to the task of analysing the way politics and philosophy were intertwined in Zilsel's work, we need to address two preliminary questions: Was he a Marxist, and was he a logical empiricist?

The answer in each case is a quite unambiguous "yes". On the one hand, Zilsel participated in discussions held by the Vienna Circle.[6] He was not present at the Paris Congress in 1935 but presumably attended the fourth Congress at Cambridge (UK) in 1938 and certainly at the fifth one Harvard in 1939. Furthermore, he explicitly endorsed the key elements of logical empiricism, including most notably:

- empiricism;
- mathematical logic as a privileged analytical tool;
- rejection of metaphysics and the endeavour to rethink traditional problems of philosophy;
- a kind of neutral monism, i.e., the construction of matter and mind out of neutral elements, to be analysed with the help of mathematical logic. (Zilsel quotes as models Ernst Mach, Bertrand Russell and Carnap [cf. Zilsel 1931a,b].)

6. On this issue cf. the discussion in [Krohn 1985, esp. 259–260].

On the other hand, Zilsel endorsed historical materialism, which he reduced to two basic theses:

- the empirical claim that there is no mental activity without matter ("physicalism");
- the methodological claim that specific cultural and intellectual phenomena can be explained as originating within the respective economic system ("sociologism").

The intention of this reframing of historical materialism is precisely to avoid conflicts with empiricism. There is indeed a twofold source of conflict between the two positions. On the one hand empiricism, with its strong anti-metaphysical strand, could be in conflict with a materialist conviction. On the other hand, empiricism was itself under suspicion of subjectivism, as is clearly shown by Lenin's attacks on Mach and "empirio-criticism" in general [Lenin 1908]. Zilsel was obviously anxious to steer clear of these disputes by eliminating all metaphysical emphasis from materialism and reducing it to "physicalism", now understood solely as a quite basic and unspecific empirical claim, in conjunction with "sociologism", which is more specific but demoted to a methodological claim. These two claims of materialism do not only differ in status but also act on different levels or scales: a micro-level for physicalism and a macro-level for historical materialism, as we shall see later. In particular, a sociological explanation of cultural phenomena can be undertaken without first having to wait for questions on the micro-level to be clarified. The way was thus open for Zilsel to convert historical materialism into sociological inquiry.

In what follows, I will look more closely at Zilsel's work, concentrating on two points where philosophy and politics seem to come closest to one another: first, Zilsel's reservations about physicalism, in Carnap and Neurath, rooted in a concern about intersubjectivism and, second, Zilsel's notion of the laws of nature, which seems to be closely linked to a progressivist view of social development.

3.2 Zilsel's remarks on physicalism in Carnap and Neurath

The first and most interesting indication of an entanglement of philosophy and politics in Zilsel's thought can be found in two texts he published in 1932, namely, a critique of Carnap in his article "Bemerkungen zur Wissenschaftslogik", published in *Erkenntnis* [Zilsel 1932a], and his review of Otto Neurath's *Empirische Soziologie*, written for the political journal *Der Kampf* [Zilsel 1932b]. In each text he expresses his concerns about intersubjectivity which he felt was under threat from, or at least in tension with, physicalism as endorsed by both Carnap and Neurath. He uses almost the same phrase in both texts to express this idea. In his critique of Carnap, we read:

> Let me mention, by the way, that the social sense also resists the asymmetry of the earlier position adopted by Carnap. It is curious: what is antisocial always proves, through careful analysis, to also be scientifically contestable.[7]

In his review of Neurath he uses the very same words, only replacing "antisocial" [*unsozial*] with "anti-socialist" [*unsozialistisch*], [Zilsel 1932b, 93]. In this proposition Zilsel seems himself to affirm a link between politics and science. Let me sketch the lines of thought in which this phrase appears.

In his comment on Carnap, Zilsel endorses an important conceptual shift that distinguishes Carnap's approach in *Scheinprobleme der Philosophie* [Carnap 1928b] from that in *Der logische Aufbau der Welt* [Carnap 1928a]: In *Scheinprobleme*, Carnap, in a solipsistic manner, takes "*das Eigenpsychische*" [i.e., elementary impressions occurring in one's own mind] as his point of departure and, on this basis, seeks to reconstruct knowledge of "*das Fremdpsychische*" [i.e., impressions occurring in other's minds]; this would eventually enable the reconstruction of knowledge of "*geistige Gegenstände*" [intellectual objects], i.e., "*Kulturgebilde*". In *Aufbau*, however, Carnap revises this view and claims that actually both kinds of occurrences, those in one's own mind and those in the minds of others, should be described in a purely physical language by so-called "protocol sentences". It is in this context that Zilsel makes the remark quoted above, which hence refers only to the solipsistic approach of *Scheinprobleme*. Nevertheless, he also had some critical remarks to make regarding the physicalist approach of *Aufbau* which are interesting for us as well. The debate about protocol sentences per se is highly relevant to our topic here, because Carnap himself retrospectively linked it to the formation of a "left wing" within the Vienna Circle [cf. Uebel 2004].[8]

Subsequent to his initial critique of Carnap, Zilsel additionally expresses severe criticism of Carnap's attempt to reconstruct all knowledge on the basis of protocol sentences. From the latter's explicit statement that protocol sentences, while providing a foundation for scientific theory, themselves lack any foundation, Zilsel inferred that protocol sentences as such were *arbitrary*. He then raises the question of how to distinguish "real" protocol sentences,

7. [My translation] "Es sei übrigens erwähnt, daß sich gegen die Ungleichmäßigkeit des älteren Standpunkts Carnaps auch das soziale Gefühl sträubt. Es ist merkwürdig: was unsozial ist, erweist sich bei sorgfältiger Analyse immer auch als wissenschaftlich anfechtbar" [Zilsel 1932a].

8. In his autobiography Carnap wrote quoted from [Uebel 2004]: "The next step in the development of our conception concerned the nature of the knowledge of singular facts in the physical world. Neurath had always rejected the alleged rock bottom of knowledge. [...] Thus some of us, especially Neurath, Hahn and I, came to the conclusion that we had to look for a more liberal criterion of significance than verifiability. This group was sometimes called the left wing of the Vienna Circle, in contrast to the more conservative right wing, chiefly represented by Schlick and Waismann, who remained in personal contact with Wittgenstein and were inclined to maintain his views and formulations" [Carnap 1963, 57].

which describe our actual world, from other sets of protocol sentences. He proposes that this could ultimately be done only by admitting what is strictly excluded by Carnap's approach: "*das Unsagbare*" [that which cannot be said], as he says, "*Erlebnisinhalte*" [actual experiences] or "*das Quale*", as Carnap translates it [Carnap 1932, 181].

Carnap responded to this objection in the same volume of *Erkenntnis* [Carnap 1932]. He accepts the task of identifying the "real" set of protocol sentences and also grants that "pure semantics", i.e., the syntax of sentences as structural elements, does not in fact provide a criterion for doing so. "Descriptive semantics", however, being concerned with "real" sentences in the sense of physical tokens (spoken words, traces of ink on paper, etc.), does so; it allows us to identify the "real" protocol sentences as those uttered by competent speakers of the respective language. There *is*, then, a criterion for a "real" protocol sentence—albeit not a formal criterion or an explicit rule, since the acquisition of language does not happen according to "explicit instructions" [*nach formulierten Vorschriften*] but "through practical methods of influence" [*durch Beeinflussung mit praktischen Maßnahmen*]. Carnap thus succeeded quite easily in refuting Zilsel's objection. It is worth noting, however, that his response to Zilsel contains one important concession: Carnap referred to the acquisition of language and thereby implicitly assumed a social context in which the competent speaker was trained. The method of protocol sentences thus relies on a social conception of knowledge.

It seems that Carnap himself was unaware of this, as can be inferred from his exchange with Zilsel. The latter persisted in his critique, making one important point which is directly related to his remark about the relationship between politics and science. Zilsel noted that one presupposition of "real" protocol sentences is indeed intersubjectivity, and that Carnap was not able to account for intersubjectivity. If in 1928 Carnap seemed to overcome the asymmetry and the solipsism of his early approach in *Scheinprobleme*, he did so only by adopting a more fundamental physical language where the original problem of intersubjectivity reappears. We have already seen that Carnap had the conceptual resources for responding to Zilsel's objection. If "real" protocol sentences are those uttered by competent speakers, and if in turn competent speakers can have acquired language only in social interaction, then real protocol sentences certainly are intersubjective. But Carnap seems not to have been aware of this: in his reply to Zilsel, he merely repeated what he had already said on this subject in *Scheinprobleme*, namely, that the intersubjectivity of protocol sentences must be taken as a mere empirical fact—as a "lucky chance", as he repeatedly said [Carnap 1931, 447], [Carnap 1932, 180]. This cannot be regarded as a satisfying response, however.

In his comment on Carnap, Zilsel confined himself to critical remarks and did not offer an alternative account that might have made it possible to explain the intersubjectivity of protocol sentences. But he at least offers some interesting clues in his review of Neurath's *Empirische Soziologie*, which he wrote at the same time. In this text, his concern about intersubjectivity

in a physicalist approach is located at a completely different level. Here, Zilsel is not concerned about the intersubjectivity of physicalist sentences, but rather fears that the abandonment of psychological vocabulary stipulated by physicalist or behaviourist approaches might diminish a person's ability to empathize with other people, to understand and share their feelings—something which Zilsel regarded as crucial for socialist engagement. It is exactly here that he introduces the phrase we already recognize from his paper on Carnap: "It is curious: what is anti-socialist always proves, through careful analysis, to also be scientifically contestable."

Zilsel is certainly aware that, in this case, unlike his criticism of Carnap, he is not pointing out a theoretical shortcoming but only a moral disadvantage. Zilsel gave his argument an epistemic turn, though, when he outlined how psychological terms might nevertheless be integrated into the behaviourist framework of Neurath's sociology. He drew an analogy to physics, where it is absolutely legitimate to refer to "forces" and "energy", even if these terms occasioned "metaphysical misuse" at times. It is sufficient for their use that utterances containing these terms can be translated into sentences about indicators of measuring instruments. (In 1932, of course, Zilsel could neither use the notion "theoretical term" nor engage in a discussion of whether these really are reducible to a purely observational vocabulary.) Similarly, as Zilsel suggested, "inner states" can be admitted within behaviourist social sciences [Zilsel 1932b, 92–93]. Although this remark is short and remains purely suggestive, some twenty years later Wilfrid Sellars proved the fruitfulness of such an approach, doing so, incidentally, precisely with a view to understanding the intersubjectivity of inner states and sense impressions.[9]

9. In his slightly later influential paper "Empiricism and the philosophy of mind", Wilfrid Sellars also proposed that "inner episodes" such as "thoughts" and "sense impressions" be introduced into behaviourist vocabulary as theoretical terms. Much more, though, he developed this idea and gave it a surprising turn, linking it again to the problem of intersubjectivity. Sellars suggested "that the ability to have thoughts is acquired in the process of acquiring overt speech" [Sellars 1956, 319]. According to the story Sellars tells, terms like "thoughts" or "sense impressions" are originally introduced into language as theoretical terms enabling a person to theorize about the behaviour of *others* and are only subsequently applied reflexively by the speaker to himself. "[T]his story helps us understand," he explicates, "that concepts pertaining to such inner episodes as thoughts are primarily and essentially *intersubjective*, as intersubjective as the concept of a positron, and that the reporting role of these concepts—the fact that each of us has a privileged access to his thoughts—constitutes a dimension of the use of these concepts which is *built on* and *presupposes* this intersubjective status" [Sellars 1956, 320–321]. Sellars thus turns the approach of *Scheinprobleme* upside down. Instead of reconstructing knowledge of "*Kulturgebilde*" from elementary personal sense impressions, Sellars shows that only by participating in culture and being socialized by our fellow human beings are we as individuals enabled to "have thoughts, sense impressions" and other kinds of "inner episodes".

3.3 Zilsel's concept of law

A second clue regarding the relationship between philosophy and politics in Zilsel's thought can be extracted from his account of the laws of nature. The concept of law is omnipresent in Zilsel's work. He was interested in this concept at a meta-level, writing about the historical origins of the concept of laws of nature and providing philosophical analyses of laws of nature and of society. In addition to this, he even tried to discover and to formulate historical laws within the framework of his socio-historical studies. He did not synthesize his numerous remarks in a systematized theory of laws of nature, however. A planned book, *The Origin and Transformation of the Concept of Natural Law*, was never realized [Zilsel 2003, xxxv]. His "account" must therefore be reconstructed on the basis of his writings as a whole. I shall seek to do this, at least in a rudimentary manner, by discussing what seem to me to be the three basic pillars of his concept of law: determinism, empiricism and, as I shall argue, pragmatism. I should say at the outset that the most striking feature of Zilsel's account is his belief in historical laws. From the standpoint of Zilsel's pragmatism, historical laws are understood as recipes for interventions rather than as descriptions of empirical regularities. In this sense, we will find key elements of Austro-Marxism (cf. the left-hand column in the table above, p. 265) at the heart of Zilsel's philosophy of science: the idea, first, that science facilitates a transition to socialism and, second, that this transition can be achieved by political interventions which put into practice scientific insights, in particular from sociology and history.

3.3.1 Empiricism

The first aspect to be noted is that Zilsel quite clearly adheres to a Humean-style empiricist account of the laws of nature. That is, he admits nothing but "facts of experience and the fact of experience itself" [Zilsel 1916, § 68] and then reduces the empirical content of laws to empirical correlations of empirical facts. It is noteworthy that from the very beginning he broadens the notion of fact to embrace social and psychological facts as well. He therefore defines laws as correlations between completely unspecified entities like "conditions", "events", or "qualities":

> Every law asserts subsistence of current association or regular connection of certain conditions and events. [Zilsel 2003, 200]
>
> The naturalist observes recurrent associations of certain events or qualities. [Zilsel 2003, 96]

In his 1926 study of the concept of genius, he explicitly applies this account to the sociology of knowledge which seeks to identify "regular links between ideas and certain states of human society".[10]

10. [Zilsel 1926, 2]: "regelmaessige Verknuepfungen von Vorstellungen mit gewissen Zustaenden der menschlichen Gesellschaft".

3.3.2 All-embracing determinism

The second characteristic of Zilsel's account of laws is an all-embracing determinism. Zilsel seemed to endorse determinism without much ado. Indeed, this was very common in Austro-Marxism and, more generally, in the Second International, where Marx's conception of history was identified with techno-economic determinism and the evolution towards socialism was portrayed as the inevitable consequence of deterministic historical laws [cf. e.g., Llobera 1979]. It was also thoroughly usual in this context to extend determinism to social life and to argue for the existence of laws of history and of society. What is special in Zilsel's work is the claim that these social laws are not reducible to physics or psychology but rather act on their own proper scale. Zilsel compares social and historical characteristics to physical macro-variables such as temperature and pressure, which also do not apply to single molecules (though they result from the action and interaction of molecules). Having provided this framework of historical laws as macro-laws, Zilsel also offers a corresponding reconceptualization of the "dispute of historical materialism":

> *If* there are historical laws, they obviously have to be macro laws. The whole controversy about the "materialist" view of history can be reduced to the question of which one is the independent and which the dependent variable, the economic structures or the cultural, religious, and artistic formations correlated with them. In macro laws, however, it is much less than self-evident that dependent and independent variables can be distinguished at all.[11] [Zilsel 1927, 286]

What does Zilsel mean when he refers to the "controversy about the 'materialist' view of history"? In my estimation, he is not referring to the question of whether there are historical laws (he explicitly assumes their existence in the first phrase, after all), but is rather hinting at a controversy about the interpretation of these laws *within* a Marxist framework. Identifying the economic structure alone as the independent variable would amount to an orthodox reading of Marx, entailing unidirectional causation from the economic basis to the superstructure. Dependent and independent variables become harder to distinguish to the extent that reciprocal action between them is admitted. This would correspond to a refined, more dialectical reading of Marx, of which Zilsel seems to approve in the quotation given above.

11. [My translation] "*Wenn* historische Gesetze bestehen, können es selbstverständlich nur Makrogesetze sein. Nun dreht sich z. B. der ganze Streit um die "materialistische" Geschichtsauffassung um die Frage, ob die wirtschaftlichen oder die mit ihnen verflochtenen kulturellen, religiösen, künstlerischen Gebilde abhängig oder unabhängig variabel sind. In Makrogesetzen aber ist es alles weniger als selbstverständlich, daß abhängige gegen unabhängige Variable überhaupt sich immer scheiden lassen."

Thoroughly unusual in terms of the standard views of the Second International—and most interesting for us—are the arguments Zilsel cites in favour of historical laws. The most important one lies in what might be called a theory of scales or of orders of magnitude [Zilsel 1931c]. According to this theory, our world can be studied on different temporal scales, each of them bearing its own typical laws and its own characteristic types of causes to be studied by different characteristic methods. On each scale, the typical speed of the corresponding processes is determined by a peculiar type of "inertia":

SCALE	REALM	SPECIFIC KIND OF INERTIA
10^{10} years	astronomy	resistance of matter;
10^{9} years	geology	resistance of matter;
10^{5} years	biology	"resistance of heredity", inertia of the genotype;
10^{4} years	history	"resistance of tradition", custom;
10^{-4} years (minutes)	psychology and physiology	reflexes and instincts, personal habits.

This theory of scales leads to what Zilsel called a "behaviouristic" definition of history:

> The realm of history comprehends human occurrences and their causes which are slower by one degree than the reactions of the individuals and faster by one degree than biological evolution. [Zilsel 2003, 217]

Besides the theory of scales, Zilsel also lends psychological and heuristic support to the idea of historical laws: he occasionally quotes the fact that Kepler not only gave an exact mathematical expression to a known regularity of nature but "succeeded in discovering the regularities in apparently most irregular phenomena" [Zilsel 2003, 112]—something which, as Zilsel intends the reader to infer, applies to society. History, as Zilsel stresses in his "Philosophische Bemerkungen" from 1929, is the most "complicated" of all lawful natural processes.[12] The task of identifying laws might thus be particularly difficult for historians, yet this difficulty differs only in degree from that of the natural sciences.

The most careful treatment of historical laws can be found in Zilsel's work on the foundations of physics, especially of statistical mechanics. For Zilsel, who identifies societies with macro-ensembles of individuals, there is

12. [Zilsel 1929, 186]: "An dem marxistischen Sozialismus [...] kann man lernen, daß die Geschichte unter allen gesetzmäßigen Naturvorgängen der verwickelste und auch Naturwissenschaft gesellschaftlich bedingt ist."

an evident link between statistical physics and sociology. Historical laws, he stresses, have to be macro-laws, i.e., relations holding between quantities which might make no sense when applied to individuals (like the notion of temperature which can only be applied to macroscopic bodies but not to single atoms or molecules). Despite being thoroughly convinced of the existence and nature of historical laws, Zilsel nevertheless carefully discusses the extent to which the necessary conditions for finding macro-laws of history are fulfilled. He identifies several points in which societies do indeed differ from ensembles such as ideal gases and which thus confirm such an approach [Zilsel 1926, 310 *ff.*], [Zilsel 2003, 202 *ff.*]:

- societies and cultures are never closed systems;
- social "ensembles" are considerably smaller in terms of the number of individuals than typical amounts of, e.g., gases ($10^9 << 10^{23}$);
- unlike atoms or molecules, members of a given society differ in activity and impact;
- social groups tend to be organized, which reduces random behaviour; the ergodic hypothesis thus does not hold;
- the scientist is of the same scale as the elements of the social ensembles, which restricts his epistemic access.

Zilsel often highlighted the fact that, as a consequence, we are still far from possessing sound knowledge of historical laws. He was bold enough, however, to outline some initial rough examples which he divided into two groups: temporal laws that describe a development, and simultaneity laws that do not contain the variable of time:

1. "Temporal laws"
   - In isolated historical systems, tribal organization precedes the beginnings of the state;
   - Individualized art and poetry are preceded by anonymous folk-art and poetry, signed paintings and sculptures by non-signed works;
   - Free artists gradually develop from craftsmen.
2. "Simultaneity laws" (time absent)
   - Wherever learned priests are entrusted with the task of teaching candidates for the priesthood, they systematize the vague and contradictory mythological traditions of the past and develop rational systems of distinction, classification, and enumeration as scientific methods.

In a review of *Die Entstehung des Geniebegriffs* Lukács pointed out in contrast that these laws actually are "very formal and therefore penetrate less deeply into concrete sensual phenomena than would have to be the case for

laws of social life".[13] He objected that these laws, as mere generalizations, were unable to uncover the driving forces of history. Much later, on the occasion of a German language publication of a selection of Zilsel's papers on the sociology of science, Freudenthal & Riethus were similarly to object that Zilsel's laws were nothing but "the sum of independent elements" and that they were unable to reveal any historical development [Freudenthal & Riethus 1977, 369]. These criticisms clearly reflect, on the one hand, the tensions between Austro-Marxism and Critical Theory, the former committed to a positivist epistemology and the latter calling for all empirical data to be interpreted in the light of a theoretical account of the reproduction of capitalist societies [cf. Schlaudt 2018]. On the other hand, the absence of a causal framework (correctly observed by Lukács) may be due less to the immaturity of Zilsel's sociological research or a fundamentally mistaken approach than simply to the fact that, as we have seen above, Zilsel thought of historical laws in analogy to the macro-laws of statistical mechanics. Boyle's law, after all, does not reveal the causal mechanism behind the link, described phenomenologically, between the volume of a gas and its pressure. Perhaps the more relevant question to Zilsel would be: What kind of underlying causal theory was he thinking of? Or, at least, what kind of causal theory would have appeared acceptable to him? Pursuing the analogy to statistical mechanics would have led him to the "atomistic" approach of methodological individualism which, however, would be unlikely for Zilsel, given the anti-individualist framework of Marxist sociology (cf. the conclusion of this paper).

3.3.3 Pragmatism

We now come to the last and most difficult aspect of Zilsel's concept of law: pragmatism. This aspect is difficult because in Zilsel we find two conflicting accounts of the meaning and nature of laws. On the one hand he takes a historical approach, favouring a pragmatist notion of laws as operational rules, while on the other he displays a systematic approach more in the spirit of logical empiricism, favouring a structural and formalist notion of law. Zilsel seems not to have made any attempt to resolve the tension that exists between these two approaches. Our task here is to do so in his stead.

Let me first spell out why I think Zilsel might have adhered to a pragmatist notion of law. There are two reasons for this. We can see some initial stirrings of a pragmatic notion of science in general in Zilsel's anthropological diagnosis that human beings interact with natural objects in a mechanical way, which may explain why, in his view, mechanics is the most fundamental of all the subdisciplines of physics:

13. [Lukács 1928, 300]: "sehr formal und dringen deshalb in das Konkret-sinnliche der Erscheinungen nicht so tief ein, wie das bei Gesetzen des sozialen Lebens der Fall sein müsste."

> Man himself is, in a certain respect, a mechanical being. All actions by which he influences the world around him consist in movements, in pushing and pulling. From the days when he learns as a baby to control his limbs, he regards that way of reacting as the only natural one. [...] [He] interprets natural processes after the pattern of human actions. [Zilsel 2003, 178–179]

Here, Zilsel grants that scientific knowledge is anthropomorphic in nature and, more specifically, that it is related to human *interactions* with nature, which amounts to a pragmatist account of science.

A further hint at a pragmatist account of science and of the laws of nature in particular lies in a historical theory regarding the origins of the latter. In the history and sociology of science Zilsel is famous for the thesis that modern science resulted from the rise of capitalism, which broke down the social barrier between rational, especially mathematical, skills on the one hand and experimental skills on the other (the "Zilsel thesis" [cf. Krohn & Raven 2000]). Previously, the two types of skills were strictly associated with two socially separated groups, academics and craftsmen, and could thus not be combined in one individual, as would be necessary for doing science.

The history of the laws of nature fits this general frame. Laws of nature owe their mathematical form to academic scholarship. As regards their empirical content, however, they can be traced back to the practical knowledge of craftsmen, especially causal "recipes" (instructions for the causal manipulation of things). Originally, then, laws of nature were "quantitative rules of operation" [Zilsel 2003, 110, 114]. This account at least suggests that, despite the surface-level descriptive semantics, laws of nature essentially have a pragmatic or operational meaning.

There is one important objection to the pragmatist interpretation suggested here. Zilsel acknowledged that, in the course of its development, the concept of law in some sense broke with its historical origins. When stated as a mathematical equation, for example, the asymmetric relation of cause and effect is transformed into a symmetrical identity based on logic [Zilsel 1916, § 98]. In *Special Relativity* and in *Quantum Mechanics*, the entirety of our knowledge is ultimately reduced to "nothing more than a set of numbers correlated in terms of a law"[14]—and thus seems to strip away all anthropomorphic and pragmatic elements.

It is Zilsel himself who draws our attention to these issues. Evidence exists, however, that for him it was not the last word on the laws of nature to regard them as purely logical and structural entities disconnected from human practice. There are two arguments in Zilsel which challenge and indeed even contradict this view, the one being more concrete and connected to experimentation in the sciences, the other more abstract and related to his

14. [Zilsel 1928, 114]: "unser Weltbild ist nicht mehr als ein Netz gesetzmaessig verknuepfter Zahlen."

overall philosophical approach. The first argument consists in the observation that in each experiment the relevant mathematical law has to be re-translated into an operational rule. The logical symmetry of quantity equations has to be broken, with one of the quantities being identified as the independent variable accessible to the experimenter's manipulations and the other as the dependent variable whose variation is finally observed in the experiment as its empirical outcome [Zilsel 1927, 281]. It becomes clear here that operational rules are not only the contingent historical origin of laws of nature but remain a relevant "mode of existence" for them besides their mathematical expression. Scientific practice involves a constant process of translation between the two modes of existence.

But there is further evidence still that Zilsel adhered to a pragmatist vision of the laws of nature. As mentioned above, this second and more abstract argument is linked to his overall view of science and knowledge, which also occasionally displays a pragmatist stance. I quote a passage from Zilsel's book *Die Entstehung des Geniebegriffs* [Zilsel 1926], where he elaborates on the sense of finding new laws of both nature and history. This sense consists for him in improving human control of the environment:

> The expansion and the acquisition of knowledge can be said to have a double meaning for the instinctual life of humans: knowledge increases and amplifies the responsiveness of man and allows him to better control his environment. Where this idea of knowledge prevails, society will aim at arranging both the relations between its members and the members' relation to nature in a more rational way; it will develop a *technique* for influencing nature and society, will predominantly direct its interest at the *future*, and its sciences will primarily aim at exploring general *laws* which also hold for the future and set out the pathways for practical responses.[15]

At first sight, it seems clear that in this quotation Zilsel is conveying a view based on historical research regarding modern societies rather than his own personal view. I want to suggest, however, that Zilsel himself subscribed to the view he describes in the above passage. There is already evidence for this interpretation in his PhD thesis *Das Anwendungsproblem* [Zilsel 1916], where

15. [My translation] [Zilsel 1926, 311]: "Die Erweiterung der Erkenntnis, der Erwerb von Wissen hat für das menschliche Triebleben wohl eine doppelte Bedeutung: das Wissen vergrößert und vermannigfacht einerseits die Reaktionsbereitschaft des Menschen und macht ihn zur Beherrschung der Umwelt tauglicher. Herrscht diese Seite des Wissens vor, dann wird die Gesellschaft darauf ausgehen, die Beziehung ihrer Mitglieder zueinander und zur Natur zu rationalisieren, sie wird eine *Technik* der Natur- und Gesellschaftsbeeinflussung entwickeln, ihr Interesse wird vorwiegend auf die *Zukunft* sich richten, ihre Wissenschaften werden vor allem allgemeine *Gesetze* aufzusuchen bemüht sein, die ja auch für die Zukunft gelten und den praktischen Reaktionen die Wege vorzeichnen."

Zilsel framed science as an integral part of a grand enterprise which he called the "rationalization" of the world. Nature as the *given*, he stresses, is purely irrational, and science attempts to "rationalize" it, where "rationalization" seems to refer to both intellectual and technical mastery over nature or, more precisely, enhanced technical control through more thoroughgoing intellectual mastery. Rationality, as Zilsel put it in Kantian terms, is not "*gegeben*" but "*aufgegeben*": it is not given but is given as a task [Zilsel 1916, §§ 127, 123]. Here again, the whole scientific enterprise is placed in the context of human interaction with, and in particular control of, the natural environment.

Many characteristics of Zilsel's philosophy thus hint at a pragmatist account of science, linking knowledge to intervention in and control of the environment. I have dwelled upon this aspect because it helps to illuminate our original question about the relationship between philosophy and politics in Zilsel's thought. The interpretation of knowledge (and scientific theory in particular) as practical knowledge, as a recipe for controlling the environment, along with a view of sociology as being key to the "social technology" of making society more rational, is indeed a central idea espoused by Austro-Marxist thinkers such as Max Adler. And although Zilsel only quotes Leibniz when explaining the idea of rationalization in his PhD thesis, nevertheless there is an implicit reference to the famous passage in the third volume of *Capital*, where Marx takes up the Leibnizian distinction between a "realm of freedom" and a "realm of necessity" and hints at the "rationalization" of man's "interchange" with nature as a collective path from necessity to freedom:

> In fact, the realm of freedom actually begins only where labour which is determined by necessity and mundane considerations ceases; thus in the very nature of things it lies beyond the sphere of actual material production. Just as the savage must wrestle with Nature to satisfy his wants, to maintain and reproduce life, so must civilised man, and he must do so in all social formations and under all possible modes of production. With his development this realm of physical necessity expands as a result of his wants; but, at the same time, the forces of production which satisfy these wants also increase. Freedom in this field can only consist in socialised man, the associated producers, rationally regulating their interchange with Nature, bringing it under their common control, instead of being ruled by it as by the blind forces of Nature; and achieving this with the least expenditure of energy and under conditions most favourable to, and worthy of, their human nature. But it nonetheless still remains a realm of necessity. Beyond it begins that development of human energy which is an end in itself, the true realm of freedom, which, however, can blossom forth only with this realm of necessity as its basis. The shortening of the working-day is its basic prerequisite. [Marx 1998, 807]

Our examination of Zilsel's account of the laws of nature brings us back to our original question regarding the relationship between philosophy and politics in his work. I will now summarize the results of this discussion and conclude with some comments on contemporary theorizing about the social role of science as viewed in the light of a Zilselean approach.

4 Scientific philosophy—a political philosophy?

Let us sum up the results of our discussion. On the question of whether or not there is a political dimension to the philosophy of science, I have highlighted two points in the work of Edgar Zilsel where political concerns seem to be directly relevant. The first is Zilsel's attack on Carnap's de facto solipsism in the method of protocol sentences. Carnap had to take as given, but could not explain, the intersubjectivity of protocol sentences. What I have sought to show is that Zilsel appears to suggest that the problem of intersubjectivity can be resolved by accounting for the monadic self in terms of its socialization. Protocol sentences are intersubjective because they are uttered by people who have acquired language in the course of their socialization. In this sense, science—like every language-based activity—is inherently social, which lends concrete meaning to Zilsel's remark that the "antisocial[ist]" coincides with the scientifically contestable. Second, I have suggested that Zilsel's pragmatist account of science relates science to technology and thus, in the framework of a progressive narrative, to both a more rational relation of society to nature and a more rational organization of society itself. In this sense, science appears beneficial to society's transition to socialism.

Assuming my interpretation is valid, does this mean that Zilsel was a political philosopher in the sense of the current debate? I do not think so, at least if we mean the idea of political philosophy as it prevails in the contemporary debate and as it appears in the following quotation from Don Howard:

> that philosophers of science, qua philosophers, [are] alert to the social, cultural, and *political implications of their work* and [see] it as a properly philosophical task to pursue those implications into the social and political arena. [Howard 2009, 201]

To be sure, Zilsel was a militant socialist with a firm ethical attitude. This attitude probably even determined his empirical programme, namely, to lay bare the social roots of science. Furthermore, he probably believed, as I have tried to show, that science is intrinsically social and is conducive to socialism. Insofar as he attempted to reveal a connection between *science*—not philosophy—and politics, I would say that, as a scholar, Zilsel was political in a

derived sense at best.[16] It may be the case that Zilsel's philosophy was based on political *premises*, but he did not put forward any political *implications* of his work in the sense indicated by Howard. He never sought to derive any normative statement of public concern from philosophical or sociological considerations, or to intervene *qua* philosopher in public debate. It is clear that the fact-value dichotomy, which Zilsel explicitly and repeatedly endorsed,[17] hindered him from doing so. George Reisch interprets the fact-value dichotomy as a strategy used by the right wing of logical empiricism to theorize its own depoliticization [Reisch 2005, 355, 364, 381]. This sounds plausible and might well be the case for Reichenbach, but it can hardly be generalized. In fact, the far left-wingers of logical empiricism had already endorsed the dichotomy prior to their emigration and were thus hindered from ever being truly political (unlike adherents to critical theory). Historically, this can be regarded as a neo-Kantian legacy within Austro-Marxism and ethical socialism, in the context of which the views of the left-wing members of the Vienna Circle had been forged.

There is one final interesting point to be made. As I stressed above, it is absolutely true that, probably due to his political convictions, Zilsel was interested in science in its social context. But it is worth mentioning that on a fundamental level his approach is incompatible with contemporary theorizing on the same topic by scholars concerned about the public relevance of philosophy and history of science. For Zilsel, the interconnections between science and society were subject to *sociological* investigation. In the sociological explanations he offers in his articles (e.g., in his famous thesis that the rise of science was enabled by the breakdown of the social barrier between "hands and tongue" (experimental and intellectual skills) in early capitalism), he refers solely to the "social and economic conditions" necessary for the rise of science, i.e., to *structural determinants*.[18] Robert K. Merton explicated the distinction between structural determinants and motives as explanations of behaviour when he defended his and Boris Hessen's view that technology and technological problems determined the fields of interest of scientists in

16. Sarah Richardson came to a similar conclusion for Carnap [Richardson 2009a, 19].

17. Emphatically in [Zilsel 1918, 196–197], and also in [Zilsel 1931b], where Zilsel put the dichotomy in an astonishingly practical context: One consequence of the fact-value dichotomy is that Marxism as an ethical conviction and historical materialism as an empirical approach are also no longer conceived as forming a whole. Zilsel underlines the utility of this distinction for the socialist party which, accordingly, can also embrace those who only share the ethical conviction but do not endorse materialism, as is the case with catholic socialists.

18. I know of only one exception in Zilsel's work, namely, his early study *Die Geniereligion* from 1918, in which Zilsel also considers psychological factors in his explanation of the emergence of the cult of the genius [Zilsel 1918, 64]. In his later work on the social origins of science he concentrates exclusively on "social and economic conditions", understood as necessary conditions for the rise of science [cf. Zilsel 2003, 3].

17th-century England. Their thesis was intended to convey the idea that technological problems acted as structural determinants on the behaviour of scientists, both opening up and limiting the horizon of scientific inquiry [Freudenthal & McLaughlin 2009, 21]. They did *not* intend to say that solving technical problems was a primary motivation of scientists. In his defense of Hessen, Merton stresses the irrelevance of motives and personal values for the sociological explanation of behaviour:

> Clark's recent critique of Hessen's essay may be taken to illustrate the confusion which derives from loose conceptualization concerning the relations between the motivation and the structural determinants of scientists' behavior. [...] Motives may range from the desire for personal aggrandisement to a wholly "disinterested desire to know" without necessarily impugning the demonstrable fact that the thematics of science in seventeenth-century England were in large part determined by the social structure of the time. [Merton 1939, 3–4]

Merton later generalized this point in his sociological approach to the study of the scientific community, now drawing a more specific contrast between motives and "distinctive patterns of institutional control" as the relevant structural determinants:

> A passion for knowledge, idle curiosity, altruistic concern with the benefit to humanity, and a host of other special motives have been attributed to the scientist. The quest for distinctive motives appears to have been misdirected. It is rather a distinctive pattern of institutional control of a wide range of motives which characterizes the behavior of scientists. [Merton 1973, 276]

As I explained above, it can be assumed that Zilsel completely agreed with this methodological framing. Scholars engaged in the contemporary debate about the public relevance of philosophy and history of science, by contrast, place particular emphasis on motives and individual values:

> Science in a social context is science influenced by values, motives, social interest, political agendas. Therefore, a necessary condition for substantive participation by philosophers of science in public debate about science in society is a theory of science that addresses the role of values and motives in science, rather than just dismissing them as irrelevant to science. [Howard 2009, 202–203]

The individualistic stance that can be observed in the above quotation is anything but an isolated phenomenon in the current debate. Helen Longino has highlighted the fact that even proponents of an alleged "social" epistemology do not renounce their individualistic presuppositions, being "resolutely and explicitly individualistic and psychologistic" and "explicitly equating social

forces with 'bias' " [Longino 2002, 54–56]. For Zilsel on the contrary, the "social context of science" could only be found in society itself, not in individuals and their motives and values. If it is true that society may sometimes occlude the scientist's view, it was in any case evident to Zilsel that the complementary thesis also holds: if a scientist is able at all to view an object, then this is due not to his inherent characteristics and qualities but to society which furnishes him with all the epistemic tools he requires. For Zilsel, social forces cannot be reduced to bias or occluding factors but also comprise the factors that enable a scientist to see.

Without addressing the question of whether one *should* refer to motives or to structural determinants instead, we can nonetheless draw the following conclusion: even if some varieties of early logical empiricism provided a model of a politically engaged philosophy of science (a notion I have contested in this paper), contemporary scholars keen to engage in public debate do *not* follow this model but have instead adopted an approach fundamentally opposed to the sociological one of the left-wing logical empiricists. This conclusion does not contradict Reisch's depoliticization thesis, however. Rather, it confirms his thesis by showing how far the discourse of philosophy and history of science has actually drifted away from its origins in logical empiricism. And it also confirms Sarah Richardson's verdict that "L[eft] V[ienna] C[ircle] scholarship carries forward a narrow framing of 'philosophy of science' " and tends to marginalize other models of politically engaged scholarship [Richardson 2009b, 170, 172].

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